india-rubber about $\frac{1}{8}$-inch thick, firmly attached by a slot and screwed bar to each roller, completes the arrangement.

The rollers being wound through about one entire revolution, and the india-rubber being thus stretched tight, layers of cloth, clay, paste or other giving material, are laid upon it. The handle is then turned in the reverse direction, and the india-rubber gradually released. Folds are in this way shown slowly growing—the broad elastic band simulating the contraction of a portion of the earth's crust. In Figs. 2 and 3, cloths are seen

FIG. 2.

FIG 3.

folded thus—first, without superincumbent weight, and second, with a weight of 30 lbs.

That the larger folds are those generated at the surface, and the smaller and more numerous those produced under pressure (*i.e.* at great depths), is here made evident.

By substituting blocks of stone or wood for ordinary weights above the cloths (Fig. 4) and repeating the experiment, some of the relations between folding and faulting are clearly shown.

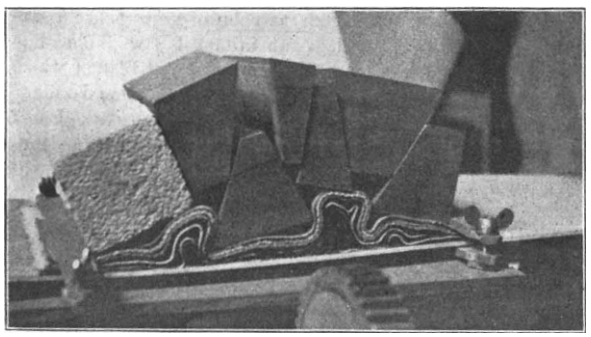

FIG. 4.

If clay be used instead of cloths, all the results of Favre's well-known experiments (*Arch. d. Sciences Phys. et Nat.*, 1878, and also NATURE), and many of those described by Cadell, Bailey Willis and others, can be obtained, and with the exercise of a little ingenuity it is easy to vary the experiments so as to reproduce a large number of the fold-forms known, and to illustrate their consequences—thrusts, faults, &c.

This machine was made for me in 1880 by the late Mr. C. D. Austen, of Newcastle-upon-Tyne, from my designs.

G. A. LEBOUR.

The Durham College of Science, Newcastle-upon-Tyne, August 18.

Scoring at Rifle Matches.

IN his letter to NATURE of August 17, Mr. Mallock appears to assume that there is such a thing as abstract "accuracy" in estimating the value of a marksman's score. The method in use at Bisley is, as I understand him, to be regarded as a rough approximation to the accurate method, whether the best available approximation or not. Is it not rather the case that the standard of accuracy is itself arbitrary, and what the authorities at Bisley have established is not an approximation to an ideal standard, but is to be regarded as a real standard of excellence?

In result Mr. Mallock's "accurate" method is this: in his notation any two scores for which $R^2+\rho^2$ is the same are of equal merit, or that one for which $R^2+\rho^2$ has the least value is the best score. Now, if "a" be the distance of any shot mark from the bull's-eye, n the number of shots, $R^2+\rho^2=\Sigma a^2/n$. Mr. Mallock's standard, then, is that the best score is that for which the sum of the squares of the distances from the bull's-eye is minimum. I see no reason why this method should be regarded as accurate *par excellence*, except the analogy of the method of least squares. But the analogy is misleading.

Where the method of least squares is applicable, the object is to find the most advantageous value of an unknown quantity to be deduced from a number of observations. An accurate value of the quantity does exist. And of two or more results deduced from the observations, that which is nearer to the accurate value is always better than one more remote, however near to the truth either may be.

In rifle shooting, on the other hand, there is generally some finite space—*e.g.* the port-hole of an enemy's ironclad, such that all shots which pass through it are of practically equal value, and all shots which do not pass through it are of little or no value.

This is much more accurately represented by the Bisley method than by the method which Mr. Mallock would substitute for it.

S. H. BURBURY.

THE only remark I should wish to make on Mr. Burbury's letter is that every shot on the target is truly the record of an observation, and that there is every reason to treat these records as far as is practicable by the methods which apply in obtaining the best means of a number of observations. Of course, it is only in the case of "centre of target" competitions the "$R^2+\rho^2$ a minimum" test applies. Prizes might well be given for close grouping, with a penalty depending on the mean distance of the group from the centre of the target.

August 22. A. MALLOCK.

Spectrum Series.

SIR NORMAN LOCKYER'S lectures on "Spectrum Series" seem to show very clearly the important fact that there is a close connection between the valency of an element and the lines in its spectrum.

The connection indicated is as follows:—

Nonvalent elements yield spectra with single lines.

Monovalents yield doubles.

Divalents yield triplets.

On turning to the list given in NATURE (vol. lx. p. 370), it will be seen that helium, by yielding doubles as well as singles, and cobalt, by yielding doubles only, are practically the only discordant cases in Sir Norman Lockyer's list, since aluminium and indium are trivalents, and their anomalous behaviour in yielding doubles only can perhaps be explained.

August 26. W. SEDGWICK.

Magnetic "Lines of Force."

IN some text-books and by some lecturers (*e.g.* Prof. A. Gray, as reported in NATURE of August 17, p. 379), the lines of magnetic force are said to be the curves along which iron filings are marshalled when sifted over a piece of card laid over a horizontally placed magnet.

Surely this is hardly correct. The true lines of magnetic force must be represented, like those of all other radiant forces, by radiating straight lines drawn through the points of action of the resultants of all the forces residing in the individual molecules of a given magnet (such points, though varying in position with the position of a magnetic body in the field, being often referred to as fixed "poles").

The symmetrical figures traced out by iron filings merely show, of course, the directions in which a line joining the poles of a very short magnet will lie in different parts of a magnetic field, under the influence of the true lines of force.

E. R. P.

August 29.

Critical Pressure.—A Suggested New Definition.

THE critical pressure of a substance is commonly defined as "the least pressure that will suffice to reduce that substance from the gaseous to the liquid state when at its critical temperature." But this definition contemplates the matter solely from the stand point of what occurs at the critical temperature, and I think it

would sometimes be an advantage to have one presenting a broader view and making no reference to any specific temperature, just as the ordinary definition of critical temperature makes no reference to any specific pressure.

Now, if in a *pv* diagram we draw the curve formed by the liquid and vapour lines, the indicator points corresponding to the "mixed state" (*i.e.* part vapour and part liquid, each more or less distinctly discernible) lie wholly within the region bounded by this curve and the axis of volume; also the ordinate of the highest point of this curve—where, of course, the tangent is horizontal—corresponds to the critical pressure, and the "critopiestic" or critical pressure line is the said horizontal tangent.

All horizontal lines below the critopiestic intersect the region corresponding to the "mixed state," while those above do not, thus showing that at pressures below the critical the substance changes from gas to liquid, or *vice-versa*, by the *ordinary* process of condensation or evaporation, *i.e.* by passage through the mixed state, while above that pressure this process does not take place, but the change occurs by continuous and imperceptible transition.

Of course all this accords with experiment, as is pointed out in several, though by no means all, the standard text-books. Thus on p. 123 of the new edition of Clerk Maxwell's "Theory of Heat," revised by Lord Rayleigh, we read:—"If we begin with carbonic acid gas at 50° F. we may first heat it till its temperature is above the critical, 88° F. We then gradually increase the pressure to, say, 100 atmospheres. During this process no sign of liquefaction occurs. *Finally we cool the substance still under a pressure of* 100 *atmospheres to* 50° *F. During this process no sudden change of state can be observed, but carbonic acid at* 50° *F. and under a pressure of* 100 *atmospheres has all the properties of a liquid . . . by this process we have caused the substance to pass from an undoubtedly gaseous to an undoubtedly liquid state without at any time undergoing an abrupt change similar to ordinary liquefaction.*"

Again, on p. 206 of the "Text-Book of Physics," by Mr. Alfred Daniell, we find:—"If CO_2 gas be exposed to a temperature above 30°·92 C. and be subjected to any pressure above 73 atmospheres, it will still be a gas: *allow it to cool, the pressure being kept up, and it will be a liquid after it passes* 30°·92 *C., and yet the transition is unobservable.*"

I therefore propose to define the critical pressure of a substance as "that pressure above which it is impossible to make the substance undergo the *ordinary* process of condensation (or evaporation)"—or if greater amplification is needed as "that pressure above which an appropriate alteration of temperature causes the substance to pass from the gaseous to the liquid state or *vice-versa*, *by a process of continuous and imperceptible transition*, and not, as happens below that pressure, by *passage through the mixed state.*"

This definition I have given in my recently published book, "Physics: Experimental and Theoretical," but the *Times* reviewer, in a paragraph in that paper of July 29, characterises it as "mere nonsense."

I shall be greatly obliged if you will publish this letter, together with your opinion on the validity of my definition. Perhaps also some of your readers may favour me with an expression of their views. R. H. JUDE.

Newcastle-upon-Tyne, August 2.

Maternal Devotion of Spiders.

ON removing some virgin cork from the wall of a conservatory a short time ago, I was much struck with the way in which a small black female spider clung to her two egg-bags, despite the fact that the piece of cork to which she was clinging had been thrown roughly to the ground. When the cork was about to be replaced on the wall, it became necessary to turn the spider adrift, in order to prevent her being crushed. But although the cork was shaken, she declined to budge, and retained a tight hold upon her precious bags. Knowing how fully alive to danger the spider race is in general, I thought that this remarkable instance of devotion to maternal promptings on the part of a naturally sensitive creature ought not to be disregarded. I accordingly removed the mother very carefully, and placed her on some rockwork, where I noticed she seemed to be very uneasy, moving restlessly about as if searching for something. I then took the egg-bags and placed them beside her. As I expected, she seemingly failed to recognise them, or at least manifested a repugnance to them, and ran away for a little distance. Subsequently, however, she returned, and proceeded to examine the bags with scrupulous care by means of her palpi; and evidently satisfied with this scrutiny that they were really her own cherished property, she commenced o spin a web about them to secure them in their place.

Rennie has described experiments with the females of certain spiders which carry about their egg-bags attached to their bodies. When one of these spiders was molested, and its bag dragged with a stick, the mother seemed to lose all sense of personal danger in her anxiety for her unhatched offspring, and fought vigorously to retain her precious egg-bag. When forcibly deprived of the bag, she manifested great distress, and commenced a search for it, and, not finding it, she refused to leave the spot, seeming to be quite indifferent as to her fate. The curious part of the story is that when the egg-bag was finally restored to her, she refused to touch it, being apparently quite unable to recognise her property. In another case the spider regained possession of the bag as it was being withdrawn, and immediately refixed it in its former position.

My spider apparently recognised her egg-bags without much difficulty, and, furthermore, seemed to be alive to the danger to which they were exposed in their new situation by her act of spinning a protecting web without delay. When evening arrived, I observed that she had drawn the bags close up under a sheltering leaf, and was guarding them closely, having placed herself between them. FRANCIS J. ROWBOTHAM.

August 23.

THE CAMBRIDGE ANTHROPOLOGICAL EXPEDITION TO TORRES STRAITS AND SARAWAK.

THE main object of the expedition was to verify and supplement the anthropological observations that I made in Torres Straits in 1888–89, with the view of the publication of a monograph dealing with the anthropology of the islanders using that term in its widest sense. A few months before leaving I received such a pressing and enthusiastic invitation from Mr. Charles Hose for the expedition to visit the Baram district of Sarawak, that I felt constrained to extend the scope of our work by accepting his tempting offer. The party consisted of Dr. W. H. R. Rivers, Messrs. C. S. Myers, W. McDougall, S. H. Ray, A. Wilkin, C. G. Seligmann, and myself.

The Torres Straits islanders are Papuans, and as they inhabit the remains of the old land communication between Australia and New Guinea it was important that they should be thoroughly studied before it was too late. The islanders have been more or less under mission instruction since 1872, and some time before then the pearl-shelling industry had commenced. Owing to the varied influences of the white man, modification was bound to take place rapidly, and unfortunately in most islands more or less extensive depopulation has also occurred. There are two distinct tribes in the archipelago—the eastern tribe inhabits the Murray Islands, Erub (Darnley Island) and Uga, and the western tribe the remaining islands. The latter people have been most under the influence of white men, scarcely a pure-blooded native exists in Erub, but the Murray Islands, on account of their remoteness and the difficulties in reaching them owing to numerous coral reefs, have been less visited. As Mer, the chief island of this group is very fertile, and has a population of some 450 people, it appeared to be the best centre for our work.

We reached Mer on May 6, 1898, and took possession of the disused mission residence, which we speedily converted into anthropological, psychological and photographic laboratories. Here we measured 63 men, 5 women, 30 boys, and 22 girls. The average height of the men is 1·653 m. (5 ft. 5 in.); their cephalic index is 77·5. Although reference is made here only to the cephalic index and the height, I may state that we usually made